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THE PRESENT STATUS OF THE ANTIQUITY OF MAN IN
NORTH AMERICA

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THE passing of that veteran anthropologist, Professor Frederic Ward Putnam, the culmination of Professor Henry Fairfield Osborn's researches in his "Men of the Old Stone Age," and Professor J. C. Merriam's recent exhaustive review in *Science* of the evidences for the early presence of man in California, all combine to make the serious reconsideration of man's antiquity in America a scientific necessity. Without in any way disparaging the valuable contributions of others, the progress so far made may be said to stand as a historical memorial to Professor Putnam, who, in spite of long years of almost barren search, still held firmly on to such faint glimpses of man's antiquity as chance threw in his way. This is not the place to record the history of these researches or to extensively review the formative work of Holmes, Abbott, Volk and others, but some characterization of the problem as it was left by these investigators is possible at this time. The extraordinary triumphs of anthropological research in western Europe naturally lead to measuring American data by these standards. When we take a comparative attitude toward the two, one striking contrast appears, for, while in Europe one finds definitely stratified superimposed beds rich in animal, human and cultural remains, in America we have little more than isolated finds, a stone implement here or a bone there. Some of these finds have called forth a surprisingly large controversial literature and still find places in our standard reference books because no satisfactory conclusion can be reached in the absence of associated faunistic or other chronological indices. Of skeletons, the celebrated Lansing man is typical. The difficulty with it is that no satisfactory faunistic or cultural associates were found and no precise way for dating the river terrace in which the bones were found has appeared. Further, the anatomical characters of the Lansing man are not distinctly different from the known American types; if they had so differed we should have had at least one argument for their antiquity.

Some writers have given undue weight to the fact that all the skeletons so far presenting claims to antiquity are of the American Indian type and so can not be ancient. This is really a fallacy, for while the presence of a distinct morphological difference would be one good argument

for a respectable antiquity, the absence of such differences would not be equally good evidence against it. Thus in Europe we find an early type known as the Crô-Magnon, which has its close parallels in the present population. Then the indisputable unity of the whole American race from north to south leaves us no choice but to conclude that its ancestral stock came from one source, and that from the start its fundamental somatic characters were delimited. There are good faunistic reasons for believing that America received its population from Asia at a period comparing to the Paleolithic of western Europe, and that since the late Neolithic has remained in almost complete isolation. According to these interpretations, there is no reason to expect that any of our skeleton finds, however ancient, will show decided differences from the surviving types. These conditions rob us of one method by which a definite conclusion could be formed as to the antiquity of random skeletal finds. About the only anatomical check we can expect is in case such finds can be identified as belonging to local historical tribes, but even here we must proceed with caution, for the great persistence of local types in Europe indicates the possibility of the long continuance of initial types in the same habitat.

European investigators have achieved their greatest triumphs from discoveries of the *débris* of human cultures, but similar deposits have not yet come to light in America. The claims of Abbott, Volk and others have been based almost entirely upon random finds of stone or bone worked by man. The American literature of some twenty years ago is curiously characterized by affidavits and sworn statements, as to the exact place, position, identity, etc., of the single objects found. This itself effectively demonstrates the insufficiency of the data. While theoretically the finding of a single stone implement in an interglacial deposit would prove the contemporaneous existence of man, its very unverifiable nature would make it of little scientific worth, and the necessity of depending entirely upon the mere assertions of an individual would leave us no recourse but extreme skepticism. In contrast to the interglacial deposits of culture *débris* in western Europe, these random American finds too strongly suggest errors in observation and natural accidents of deposition to afford a basis for any kind of scientific or mere speculative structure.

It is true, of course, that the mere accumulation of these observations, isolated though they be, adds greatly to their weight and will, if long continued, give a sufficient basis for some kind of an interpretation. The only part of the country from which such an accumulation begins to appear is the Pacific coast of the United States, where Professor Merriam cites at least eleven different finds in Pleistocene and earlier formations. But notwithstanding the obvious fact that, given enough time, a very respectable series of artifacts may come to hand from the gravels of the Pacific slope, the difficulty of correlating them and of interpreting the

culture of the period is still very great. The experience with European deposits is decidedly against the successful use of such isolated finds. It seems a safe prediction that unless the original culture deposits can be found in the Pacific coast area, faunistically or otherwise dated, we shall always find it necessary to maintain a more or less skeptical attitude toward the antiquity of these isolated finds, even though they be of great number.

One difficulty we have not yet stated is analogous to that encountered in skeleton finds: viz., the lack of distinct differences between these supposed antiquities and objects found on the surface of recent village sites. As in case of skeleton material, the fact that certain differences can not be observed is no good reason for doubting their antiquity; it simply leaves us with an even chance that their presence in all geological strata is intrusive. The crucial point is that, notwithstanding the vast amount of searching by archeologists, amateur and otherwise, nowhere within these United States and Canada have we record of a culture deposit that clearly precedes or accompanies the last period of glaciation. As to what the future may bring forth no prediction is safe. It is probable that if the auriferous gravels of California do have human relics, stratified deposits will ultimately appear. This part of the country is still but newly occupied and may in the near future reveal rock-shelter deposits of undoubtable antiquity. On the Atlantic seaboard and in the limestone cavern area of the Mississippi Basin, there is much less reason to expect new discoveries, for the ground has been worked over with considerable care. The only site about which even a strong presumption of antiquity can be raised is at Trenton, New Jersey, where Dr. Abbott, and later Putnam and Volk, made certain observations, upon the basis of which a theory of stratification has been advanced. The Trenton site presents first a black sandy soil of some two feet in depth, below which is a layer of yellowish sand from one to three feet and resting upon gravels. In the black soil were remains clearly identified with those of the Delaware Indians, formerly occupying the vicinity. In the sand disconnected with the soil were random stone implements. By dint of long and patient search Mr. Volk discovered in the gravels beneath two fragments of human bones and one or two problematic pebbles, suggesting human workmanship, but not positively. On this basis Putnam assumed three culture periods for the Delaware Valley, the oldest preceding the last advance of the ice, the second during the final retreat of the ice, and lastly the historic period.

It will be noted that the data for the first period consist of little more than a single find and, as may be expected, have been looked upon with extreme skepticism. No faunistic associates were found in the gravels except a fragment of a scapula, which may be musk ox, though it can not be positively distinguished from bison or even domestic ox.

Altogether these are not adequate data for establishing a culture period. Fifteen years of subsequent observation by Mr. Volk have failed to bring anything more to light, and also, since it is not clear that the gravels in which these bones were found were undisturbed by post-glacial floods, we must consider the case as far from proved.

This brings us to the middle sand layer, which enjoys the distinction of being the only archeological find so far reported that has a fair claim to antiquity. Mr. Volk ran a series of trenches in one part of the site, discovering some fragmentary human bones and a number of stone implements. The variety of artifacts indicated a cultural difference from those found in the black soil above and as they appeared to be made from argillite, the assumed culture was designated as Argillite culture. After this definite result Mr. Volk did not trench further, but rested his case.

Mr. Volk's claim for the antiquity of this layer was generally rejected on assumed geological grounds. In brief, it was regarded as a wind deposit of comparatively recent formation, most of the artifacts having merely "settled" down from the black soil above. Failure to meet either of these objections would waive all just claim to antiquity. As no further trenching was reported, the case necessarily stood as not proven and interest in the site was lost.

After an interim of about ten years, Dr. C. C. Abbott, owner of the site and its original discoverer, invited the American Museum of Natural History to reexamine the deposit. This resulted in a more systematic investigation which gives us data of a more positive kind. The trial trenching was carried on by Mr. Alanson Skinner and the stratigraphic work by Mr. Leslie Spier. The report of Mr. Spier fully agrees with Mr. Volk's observations, but presents some new problems. Mr. Spier discovered that the vertical distribution of the artifacts in this yellow sand took the form of a normal frequency curve. It was then possible to compare with precision the series from the various trenches and also with the former data of Mr. Volk. By this triumph of method it was shown that through the deposit extends one plane of maximum frequency around which the artifacts fall in a normal frequency curve. Pebbles of appreciable size were found in the sand and two series for these showed that they also had the same frequency curve with approximately the same average. In other words, the series for artifacts is the same as the pebble series, the former being but worked pebbles. Hence, the same agency that deposited the pebbles also deposited the stone implements. While man could have carried the artifacts and dropped them on a wind-growing sand dune, it is inconceivable that he should have carried the pebbles, for they are many times more numerous than the worked stones. Further, a recent study of known wind and water deposits by a geologist shows that the normal frequency curve for

coarse grains of sand is characteristic of water deposits, but not of wind-carried material.

Therefore, the archeological brief for this case may be said to be in. It shows artifacts of stones deposited in a definite way agreeing with our present knowledge of water deposits. These artifacts are distinct from and below the cultural débris of the Indians living here at the discovery of America, and are therefore older. The age of the deposit is a geological problem which awaits further investigation, but as a water deposit it must be not later than the last recession of the ice from Northern New Jersey.

While this case has thus come to a definite pass and stands so far as the best evidence for an ancient culture in America, it still presents some of the characteristics noted at the outset. It also is in a sense a random deposit of isolated specimens, selected and carried by a natural agency. Under such conditions the cultural association of the objects can not readily be determined, and until a camp site of this culture is uncovered and identified there will be a general hesitancy to accept the result noted above. The case of the Delaware Valley is thus about the same as that for the Pacific coast, and in contrast to the condition in western Europe, where we have definite camp deposits, is rather disconcerting. At least it presents a serious problem that calls for new methods and far less slipshod field-work than some of our professed anthropologists have perpetrated in the past. On the other hand, local collectors in every part of the country should be encouraged to put their discoveries on record, even their isolated finds, and to examine all cuts and excavations in the old river terraces of their respective communities, because the accumulation of even isolated finds may ultimately pilot us to the long sought original deposits. As in Europe, the solution of the problem will depend upon the joint efforts of individuals residing in many localities and not upon the work of a single individual.